Response-to-Comments Document

for the

February 2002 Peer-Review Draft of the American River PHABSIM/2-D Flow Study Report

February 2003

AUTHORS RESPONDING TO COMMENTS

U.S. Fish and Wildlife Service

Mark Gard

PREFACE

This document contains the comments provided by scientific peers on the February 2002 draft of the report, "Comparison of PHABSIM and 2-D modeling of habitat for steelhead and fall-run chinook salmon spawning in the Lower American River" (Report), and responses to those comments. This compilation is divided into subject-matter sections whereby various comments and responses to authors were organized. To the extent that individual comments crossed over subject matters, the author collectively addressed those comments.

In addition to the individuals identified on the following page, the Report was also provided for peer review to Craig Addley, Utah State University, who did not provide any comments on the Report.

Although this compilation may provide useful insight into how the comments were addressed by the author, the Report itself represents the complete and final synthesis of studies on the comparison of PHABSIM and 2-D modeling of salmonid spawning in the Lower American River, based on the best available scientific information. The author has reviewed his responses and compared them to the final Report to ensure that all comments have been adequately addressed. To the extent that any discrepancies remain, the Report itself should be viewed as the final statement.

Lastly, the author of the Report wishes to thank everyone who provided comments on the February 2002 draft. The comments greatly assisted the author and agency in identifying missing or unclear information, focusing the textual and graphic presentations, and thereby producing a better overall Report.

LIST OF PEER REVIEWERS

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LIST OF ACRONYMS

2-D	Two-dimensional
ADCP	Acoustic Doppler Current Profiler
ASCII	American Standard Code for Information Interchange
BRM	Bed Roughness Multiplier
CDFG	California Department of Fish and Game
CVPIA	Central Valley Project Improvement Act
FWS	US Fish and Wildlife Service
HSC	Habitat Suitability Criteria
HSI	Habitat Suitability Index
IFIM	Instream Flow Incremental Methodology
PHABSIM	Physical Habitat Simulation Model
RHABSIM	Riverine Habitat Simulation Model
TIN	Triangular Irregular Network
USGS	US Geological Survey
WSEL	Water Surface Elevation
WUA	Weighted Useable Area

GENERAL COMMENTS

Terry Waddle

Comment 1: Soundness of study design: The study design in this report is logical and appropriate to the problem. Comparison of the results from two different modeling paradigms requires controlling as many variables as possible to ensure the two modeling approaches are applied to the same problem. By confining the model applications to exactly the same portions of the river, you have achieved the needed level of control.

Response: No response needed.

Comment 2: Clarity of objectives: A specific statement of objectives should be included in the report prior to the Methods section with a primary level heading similar to Introduction and Methods. In it you should state what kind of comparison(s) you wish to make and how you will know they were successful or not successful. This will give the reader background relevance while reading the methods section.

Response: We do not feel that the addition of an objectives section is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes. The introduction states the objective of the study: to produce models predicting the availability of physical habitat in the Lower American River for fall-run chinook salmon and steelhead spawning over a range of stream flows.

Comment 3: The report needs a discussion/conclusion section. There is some discussion in the Results section, but an expanded discussion of the simulation results, especially for the depth and velocity simulations, is needed. In such a discussion, the objectives should be reiterated and a statement made about whether each objective presented in the initial objective statement was met.

Response: We do not feel that the addition of a discussion section is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

Comment 4: Technical soundness of methods: The methods chosen are technically correct to the problem of comparing results from the two different modeling paradigms.

Response: No response needed.

Comment 5: Adequacy of data: In the report you acknowledge that some of the simulated flow phenomena occur in areas where more topographical data would have removed ambiguities. This is indeed the situation. Most of the raw data you have is based on cross sections that are connected by breaklines. This results in some uncertainty as to the bathymetry in the area between cross sections. In most alluvial river channels the bathymetric features tend to be longitudinal due to the nature of sediment transport phenomena. However, there are sufficient exceptions to this rule of thumb to indicate the necessity of collecting data either longitudinally as well as laterally or using very close spacing lateral transacts (1/10 channel width or less) to fully capture the bathymetry. This said, I do not think the overall results would be significantly affected by obtaining new data, though local changes such as better distribution of discharge at the inflow and resolution of some eddy areas may be obtainable.

Response: In general, we spaced our lateral transects at approximately one-third channel widths (see plots of study sites in Appendix C). The methods that we used to collect the deep bed topography data (with ADCP) required that we collect the data laterally, since we needed the WSEL to be the same for all points on each ADCP line to be able to calculate bed elevations from the measured depths. In general, it appeared that the spacing we used captured the bathymetry of the channel, since we were able to track channel features, such as thalwegs, from one cross-section to the next. While we agree that the methods used did result in some uncertainty as to the bathymetry between cross-sections, we feel that the data we collected generally did capture the bathymetry of the study sites.

Comment 6: Validity of the findings: One conclusion that you do not state stands out: The habitat suitability criteria (HSC) are a much more significant factor in determining the outcome of a habitat study than the type of hydraulic/hydrodynamic model used. Similarly, different hydraulic simulation options w/in PHABSIM have less influence on study outcome than HSC.

Response: We agree that HSC are a much more significant factor in determining the outcome of a habitat study than the type of hydraulic/hydrodynamic model used. However, we attribute most of the errors in simulating the combined habitat suitability of redd locations to the hydraulic/hydrodynamic models used, rather than the HSC.

Comment 7: Your finding that the habitat study outcome varies only a small amount between the two flow model types is expected. I do not think the data and study results present a strong enough argument for the 2-dimensional approach over the 1-dimensional approach to establish the 2-dimensional paradigm as policy for future studies. Rather, I do think the conclusion can be drawn that the two model paradigms produced sufficiently similar results that choice of the flow model to use should be based on cost-effectiveness of the flow model application without regard to the HSC.

Response: The addition of site-by-site graphs of flow/habitat relationships in Appendix E, showing much greater differences in habitat outcome between the two flow model types, make a much stronger argument for the 2-dimensional approach over the 1-dimensional approach.

Comment 8: Clarity of presentation: I got somewhat confused in the presentation of results and conclusions. Please see specific remarks about that section below.

Response: The results section has been revised to improve the clarity of the presentation of results and conclusions. See responses to Results comments.

Comment 9: Adequacy of figures and tables: Generally, the figures and tables presented in the text supported the points made. Tables 2 and 3 could be combined to give the reader one place to recognize the substrate, bed roughness, and particle type coding. I would like more discussion of the items in the appendices to both understand how the tabular information was derived and how it was interpreted. (See earlier comment about expanded results discussion.)

Response: Tables 2 and 3 have been combined. The discussion of the items in the appendices, both related to how the tabular information was derived and how it was interpreted, is given in the methods section.

Comment 10: Need conclusion section: Support or revise flow recommendations of earlier work

Response: We do not feel that the addition of a conclusion section is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

Peter Steffler

Comment 1: Overall, I found the report to be very interesting. I think the study was executed well and as such it forms an important step in the validation of 2D methods. As such, I think it could and should be published. The hydrodynamic comparison was interesting enough but the habitat comparison was particularly fascinating. Maybe this is because I don't know very much about it to begin with.

Response: No response needed.

Comment 2: Site length and inflow conditions: Some of your sites are quite short and in all cases, the study area coincides with the area covered by real topographic data. I'm not sure if we say it anywhere, but roughly one channel width from the inflow boundary is susceptible to error. Setting an artificial inflow helps, but if it does not correspond to the real bathymetry, then there can be large velocity errors, as you noticed at several sites. I suspect that it is present at sites, just to a lesser degree. The same is true at downstream boundaries, although not as noticeable. One answer is to take more topo data and move the computational boundaries further away from the area of interest. Another is take to take a detailed velocity distribution across the inflow. Unfortunately, this changes with discharge. In any event, the extra work has to be factored into the study cost. I think I knew that you added a one channel width extension. This helps, but my point (not so clearly made) was that the model is not reliable until you are a channel width or so into the real topographic data.

vel data

Response: We agree that the collection of bathymetry data upstream of the study sites could improve the simulation of conditions in the uppermost portion of the sites, but would have added to the cost of the data collection. In the current study we are collecting data on (on the Yuba River), we have obtained bed topography data from the Army Corps of Engineers that we will be using for the area upstream of the study sites. We measured detailed velocity distributions across the inflow, and suggest in the report that this data could have been used for the Sailor Bar site to improve the velocity simulation for that site.

Comment 3: Field data program: Some idea of time spent one each task, or the productivity would be interesting. Also, it would be interesting to report the number of data points for site and what proportion was collected by the two different methods between the transects. Later, in the validation and results, you could consider the effect of the "quality" of the field data on the results. In particular, the difference between wading and boat gathered data is interesting. Did it correlate with the zero suitability error in the habitat validation?

Response: Data on the time spent on each task is included in a separate manuscript which we have recently submitted to the North American Journal of Fisheries Management. We added a table to the report showing the number of data points for each site collected with each method. It is unlikely that the wading versus boat gathered data related to the zero suitability error, since almost all of the zero suitability error was due to inaccuracies in substrate, and for both methods the substrate data was gathered in essentially the same manner (with visual observation).

Comment 4: Roughness Calibration: The issue of roughness calibration is still very much in its infancy and your approach and results represent an important step. There are a couple of things here. One is the magnitude of the BRM's that you used. Starting with five times the observed bed size is probably reasonable. For many sections, the final value was within an order of magnitude (BRM 0.3 to 3) which might be explained by uncertainties in the bed roughness. Eg. higher roughness for more angular, more non-uniformly graded material. Lower roughness for more rounded particles, more narrowly graded, more emdedded (paved). The very large (or small) BRMs are another matter. The main reason for an order of magnitude increase in k would be that the principle resistance is form drag rather than skin friction. Bedforms such as dunes and bars might be responsible, as well as obstructions such as boulders or vegetation. Very small BRM's (0.01) can only be explained by topographic anomalies. Somehow, an artificial constriction is causing the water to pond at a greater depth than it should. The only way to lower the water is to increase the flow area available, which means lowering the bed elevations at the control. One thing to keep in mind is that the origin for the bed level is somewhat ambigiuous in the presence of large roughness, but that it should be close to the bottom of the roughness. Therefore, the rod placement should tend to be between roughness elements, rather than on top. Again if observations can be correlated to the BRM's it would be very interesting.

My usual advice on roughness calibration is to apply a single multiplier to each substrate class, even over a number of sites. This is fairly easy to do with global search and replace in the .bed files. The number of multipliers is limited, and the range of possible variation is also limited. The number of test criteria for each multiplier is also increased. It is more difficult to enforce the criteria objectively, however. I think that there is a better chance of having good results at non-calibration flows this way. It also serves to identify anomalous areas where roughness calibration is not effective.

Response: While we agree that it would be good to limit BRM's to the range of 0.3 to 3, time and manpower constraints make it impossible for us to modify and rerun the models. In future studies, we will restrict our BRM's to the range of 0.3 to 3. We feel that bed roughness represents both roughness of particles and variations in bed topography that are smaller than the scale of the model. We have been reluctant to change bed elevations, viewing that as a tradeoff between accuracy in bed elevations versus accuracy in simulated WSELs. While it might be more accurate for the model to measure bed elevations close to the bottom of bed roughness elements, it is inconsistent with previous practices in instream flow studies in measurements of bed elevations, and thus we feel that we should maintain our normal practice where bed elevations are measured at whatever location the rod is placed at. In addition, this method would produce greater depths, which would tend to bias discharge measurements high. For collection of ADCP data, it is not possible to collect bed topography data in the method suggested by the commenter. We do not feel that it would be practical to apply a single multiplier to each substrate class over a number of sites, since all of the substrate class multipliers would need to be changed simultaneously for all sites until all of the sites calibrate. Also, this method would not take into account site-specific variations in bed topography that are smaller than the scale of the model.

Comment 5: Recommendations for 2D data collection, use of 2D models, and development of 2D models: The report does not address any of these, but I think your recomendations would be valuable to users and developers if you were to develop this into a paper.

Response: While we agree that this information would be valuable if this report was developed into a paper, we do not feel that the addition of this information to this report is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

Comment 6: Recommendations for future validation studies: Similarly, your thoughts would be very valuable. You are one of the first to do a validation study. What would you do differently next time? Working toward some sort of standard approach is highly desirable. The problem that most model developers have is that their engineering background leads them to be satisfied with artificial tests, even with comparison to simplified analytical solutions. Real world validation is much more difficult.

Response: While we agree that this information would be valuable if this report was developed into a paper, we do not feel that the addition of this information to this report is warranted, given that the purpose of the report is to provide additional scientific information to the FWS CVPIA staff, who are responsible for implementing any revised flow regimes.

PREFACE AND INTRODUCTION

Terry Waddle

Comment 1: Usually "final report" is a summary of previous efforts & contains final conclusions. Is that true of this report or is the last of a series? Change "investigations" to "studies." (preface).

Response: "Final report" has been changed to "last report." The remaining change has been made.

Comment 2: Change "this" to "that" in first sentence. Change "This" to "The 1996" in second sentence. "CDFG" abbreviation not previously identified, spell out first use. Spell out first use of abbreviations "PHABSIM" and "2-D" - change text to Physical Habitat Simulation System (PHABSIM) and two dimensional (2-D) (Page 1, Paragraph 2).

Response: CDFG was identified in the preface. The remaining changes have been made.

METHODS

Terry Waddle

Comment 1: Change "Study Site Selection" to "Background." (Page 1, between Paragraphs 2 and 3).

Response: The suggested change has been made.

Comment 2: The River2D program developed at the University of Alberta was largely funded by Fisheries and Oceans, Canada and by the Alberta Department of the Environment. Instead of mentioning funding here, it would be best to say something like: "We decided to run both PHABSIM and the 2-dimensional modeling program River2D developed at the University of Albert, Edmonton, Alberta. This model is currently being used by the USGS office in Fort Collins, Colorado for habitat studies." Funding not relevant to your report. Instead of discussing funding, perhaps it would be better to say "developed at the University of Alberta, Edmonton, Alberta and used by the USGS office in Fort Collins". FYI - the Canadians have funded development of this for over 10 years and millions of $ not US. (Page 1, Paragraph 3)

Response: The suggested change has been made.

Comment 3: Add heading "Models used." (Page 1, between Paragraphs 3 and 4)

Response: The suggested change has been made.

Comment 4: First use of the abbreviation "IFIM." Change "are" to "can be" in second sentence. (Page 1, Paragraph 4)

Response: IFIM has been defined. The suggested change has been made.

Comment 5: You should say more about PHABSIM to give the new reader a balanced view of what you get from each model. The subsequent pages give an extensive justification for the 2-D model and its capabilities. In this paragraph you should give at least a rudimentary development of PHABSIM including that it is 1-D, based on extrapolating cells from transects, that the velocity simulation is an empirical process calibrated to the study site, and explain the basic WUA calculation process. Then in the 2-D development you should be sure and say that the same WUA concept is used, only that the model used to develop depth and velocity is different. PHABSIM: 1-D, Transects, Extrapolate for cross-section to intermediate area, Velocity simulation: empirical, HSC & WUA same concept for both models. (Page 1, Paragraph 4)

Response: The suggested change has been made.

Comment 6: You state that bed topography, substrate, water surface at the bottom of the site, are used to predict habitat. More is required for habitat than just those items. This is where a better development of the WUA calculation process in the PHABSIM description could be referred to appropriately. The whole HSC idea needs to be introduced somewhere and the distinction between flow models and the similarities in their use to calculate microhabitat made more clear. Add "Similar to one-dimensional PHABSIM" to first sentence. Add "However," and "common to PHABSIM" to second sentence. Add "in complex channels" and "empirical" and delete "and bed roughness" from third sentence. Change "is small enough" to "of resolution can be adjusted," delete "discrete" and add "extrapolated from transects" to fourth sentence. Change "quickly" to "rapidly" in sixth sentence. Change "edge" to "randomly sampled" in seventh sentence. Add "when coupled with additional software" to last sentence. (Page 2, Paragraph 1)

Response: "HSC" has been added to the first sentence to acknowledge that more is required for habitat than bed topography, substrate and water surface elevation. We disagree that "and bed roughness" should be deleted, since bed roughness is one of the parameters used by the 2-D model to predict velocities. The idea of HSC is introduced in the Habitat Suitability Criteria section of the methods. A sentence has been added to the end of this paragraph making it clear that the methods used to compute microhabitat are the same in both models. The remaining suggested changes have been made.

Comment 7: Add heading "Study site selection." (Page 2, between Paragraphs 1 and 2)

Response: The suggested change has been made.

Comment 8: It is not clear whether you calibrated the 2-D model to the results of the 1-D model or to original data and why. Particularly the "why" needs elaboration. I think you calibrated to data and used the stage-discharge function of PHABSIM to generate other starting WSL values for simulated flows in the 2-D model, but you don't tell us that important bit of information. Better separation of calibration and simulation phases is need. (Page 3, Paragraph 2)

Response: This paragraph has been modified to clarify that, for this study, the measured WSELs were used to calibrate the 2-D model. For this study, WSELs were measured at a flow slightly higher than the highest simulation flow; measured WSELs were used since they would be more accurate than WSELs simulated by PHABSIM. In other studies, where the highest simulated flow is much greater than the highest flow at which WSELs were measured, we have calibrated using the WSELs simulated by PHABSIM, since we feel that any inaccuracies in the PHABSIM simulated WSELs are more than countered by the increased accuracy of calibrating the 2-D model at the highest flow to be simulated. A sentence has been added to clarify the differences between the calibration and simulation phases.

Comment 9: Change "enough" to sufficient in third sentence (Page 3, Paragraph 5).

Response: The suggested change has been made.

Comment 10: Change "There were two techniques" to "Two techniques were," "by taking shots from" to "using" and "to" to "and" in first sentence. Change "run" to "traversed" in second sentence. Change "down" to "along the longitudinal gradient at," add "longitudinal" and delete "down the site" from last sentence (Page 3, last paragraph).

Response: We have decided to stay with "run" instead of "traverse" for brevity and to be consistent with our previous reports. The remaining suggested changes have been made.

Comment 11: Change "runs" to "traverses" and "run" to transect (Page 4, Paragraph 1).

Response: We have decided to stay with "run" instead of "traverse" or "transect" for brevity and to be consistent with our previous reports.

Comment 12: Delete "in order" in first sentence. Start new paragraph before last sentence (Page 4, Paragraph 2).

Response: We have decided not to split up this paragraph since the end of the paragraph still refers to shallow redds. The other suggested change has been made.

Comment 13: Change "above" to "previously described" in first sentence. Change "runs through" to "traverses of" in second sentence. Change "taking a shot" to "sighting" and add "located on the boat" to the fourth sentence. (Page 5, Paragraph 1).

Response: We have decided to stay with "run" instead of "traverse" for brevity and to be consistent with our previous reports. Adding "located on the boat" would be inaccurate, since the stadia rod and prism were on the boat only for deep redds; for shallow redds, the person wading was holding the stadia rod and prism. The remaining suggested change has been made.

Comment 13: Add "(See Table 1)" to first sentence (Page 5, Paragraph 2).

Response: The suggested change has been made.

Comment 14: Tell the reader the difference between PHABSIM and RHABSIM; else they may think you just missed a typo. Always spell out the item at the first use of an abbreviation (Page 6, Paragraphs 1 and 2).

Response: The suggested changes have been made.

Comment 15: Change "run" to "traverse" in first and second sentences.

Response: We have decided to stay with "run" instead of "traverse" for brevity and to be consistent with our previous reports.

Comment 16: This paragraph appears to say that you input and output the same data. Why was it necessary to do so? Why not just use the data without this step? Tell us why this was important. (Page 6, Paragraph 2)

Response: This paragraph has been modified to clarify that the first ASCII file contained the raw data, while the second ASCII file contained the bed elevation, average water column velocity and station data. Thus, the RHABSIM step is a data-processing step. The raw data has northing, easting and vertical velocity components for each cell going down through the water column and depths for each of the four beams. RHABSIM produces the average water column velocity perpendicular to the run, computes the station that is perpendicular to the run, and computes the average depth for the four beams, for each measurement (ensemble).

Comment 17:. You deleted data that fell outside your acceptable range. Do you have a reference suggesting this is legitimate or appropriate? What does the ADCP manufacture's handbook say? Other sources? If you do not have other sources, the reader needs to know that the values that failed this test were considered outliers or artifacts of the sampling process and were discarded because they would skew the reported values inappropriately. (Page 6, Paragraph 2)

Response: We do not have any references to this technique. The ADCP manufacturer's handbook does not discuss any such techniques, nor have we found any other sources on this issue. We have added a sentence as suggested in the comment to support that this technique is legitimate and appropriate.

Comment 18: Add "longitudinal," change "down" to "of" and change "distance down the site" to "longitudinal location" in first sentence. Change "run" to "traverse." Delete "along the run" in third sentence. Add "(position)" to four sentence. Add sentence at end of paragraph: "The ADCP traverse positions and the PHABSIM transect positions were referenced to the same horizontal and vertical benchmarks described earlier." (Page 6, Paragraph 3)

Response: We have decided to stay with "run" instead of "traverse" for brevity and to be consistent with our previous reports. The remaining changes have been made.

Comment 19: Reference the brand here or put documentation in references and cite here (Page 6, Footnote 3).

Response: A reference to the brand of ADCP has been added where the ADCP is first mentioned (on page 3).

Comment 20: Change "runs" to "traverses" in first sentence. Change "stable" to "distributed by the model" and change "site" to "study area" in third sentence (Page 7, Paragraph 2).

Response: We have decided to stay with "run" instead of "traverse" for brevity and to be consistent with our previous reports. The remaining changes have been made.

Comment 21: What is the basis for using 5 times the average particle size? A reference is needed, even if it is a personal communication from the model author. (Page 7, Paragraph 2)

Response: A footnote has been added regarding the basis for using five times the average particle size.

Comment 22: Combine tables 2 & 3, you have room.

Response: The suggested change has been made.

Comment 23: Readers will not know what a breakline is. It would be better to say something like: "A utility program, R2D_Bed, was used to refine the raw topographical data TIN (triangulated irregular network) and define the study area boundary. The bed topography of the sites is shown in Appendix A." (Page 8, Paragraph 1)

Response: We felt that it was necessary to include details on the derivation of the final bed files, since we have gotten comments in other peer reviews that our reports did not include sufficient details. A footnote has been added defining what a breakline is.

Comment 24: Because River2D uses solely metric units, I think you should show metric values in parenthesis along with the imperial units (Page 8 and throughout the report).

Response: We have added metric values in the portions of the text that refer to River2D, but did not add metric values to the tables, since they would have made the tables more confusing.

Comment 25: This paragraph should only say that a computational mesh and the required input to the River2D model were prepared using a utility program, R2D_Mesh. Cite the model documentation for both the bed and mesh program. You do not need to reproduce them here. This detail should be in appendix or in a cited reference.(Page 8, Paragraph 2)

Response: We felt that it was necessary to include details on the derivation of the mesh, since we have gotten comments in other peer reviews that our reports did not include sufficient details. In addition, we felt that the details on the mesh program were needed to place the results in the last half of the paragraph into context. We felt that the information in this paragraph was important enough to be included in the text, rather than in an appendix. A footnote has been added defining what a mesh breakline is.

Comment 26: The first sentence should be modified to read "The RIVER2D software was used to compute the depths, velocities and WSELs throughout the site." The beginning of the second sentence should be modified to read "The model was run to steady state." Here again there is confusion between calibrating to the measured data and using the 1-D model to provide stage-discharge values at the downstream end of the site. If you did calibrate the 2-D model to the results of the 1-D model, you need to explain why. (Page 8, Paragraph 3)

Response: We have not modified the first sentence, since we felt that the details in the sentence are needed for readers to understand what the RIVER2D model uses as inputs to compute depths, velocities and WSELs. The suggested change to the second sentence has been made. See response to comment 8. This paragraph has been modified to clarify that, for this study, the measured WSELs were used to calibrate the 2-D model.

Comment 27: You are giving very specific criteria for a successful solution in a single particular model. These criteria need either to be cited from another source or explained in detail if you derived them yourself. Change "net flow" to "net outflow" in first sentence. Delete "still" from fifth sentence. Delete "of" from sixth sentence. Add "for low gradient streams" to second sentence. (Page 9, Paragraph 1)

Response: Citations have been added for the sources of the criteria. The remaining suggested changes have been made.

Comment 28: I suggest you ask the model author how to explain the high Fr in shallow area. This is an artifact of the River2D calculations and needs an explanation in terms of the River2D model. Was this related to gradient/topography? (Page 9, Paragraph 1)

Response: The model author (Peter Steffler) responded: "The answer comes from a simple error analysis. The model computes depth and discharge intensity as its primary variables. Velocity and Froude # are derived variables so errors in the primary are propagated through the derived calculation. The calc for Froude # is F = q/(g^(½)*D^(3/2)). So the error in F can be estimated by sF^2 = sq^2/(g*D^3) + sD^2*(9/4)*q^2/(g*D^5) where sF is the expected variance in F etc. You can play around with this formula a bit, but the conclusion is that sF can be very large when D is small. I think the first term dominates in most cases. For example, if D = 0.01 m and sq = 0.001 m^2/s and sD = 0 (very good model accuracy), then sF = 0.3 or so." Thus, we conclude that the Froude numbers greater than one are a result of small errors in depth. The occurrence of Froude numbers greater than one did not appear to be related to gradient or topography.

Comment 29: Is tabular data available [on the differences between banks for WSELs mentioned in the fourth sentence]. Change "get" to "adjust" in the seventh sentence. Change the following [eighth] sentence as shown: "Since varying the BR Mult across a transect does not generally work to calibrate WSELs, we viewed the calibration in these cases as acceptable because the difference between observed and predicted WSL was similar to the variation in WSL measurements obtained on opposite sides of the channel." Add "for," change "get" to "obtain," delete "on" and change "pulled down" to "depressed" in the ninth sentence. Add "measured" to 12th sentence. (Page 9, Paragraph 2)

Response: The following table shows differences between banks for WSELs for Above Sunrise XS6, Sunrise XS7 and Rossmoor XS3. We didn't feel that it was necessary to add this table to the report. For split channels, only one WSEL was measured (near the bank). We have not changed "get" to "adjust" in the seventh sentence, since that would make it sound like we adjusted the predicted WSEL, when we actually adjusted the Bed Roughness multiplier. The remaining suggested changes were made.

Site	XS	Flow (cfs)	LB WSEL (ft)	MC WSEL (ft)	RB WSEL (ft)
Above Sunrise	6	7,512	88.41	---	88.59
Above Sunrise	6	11,175	88.20	---	88.43
Above Sunrise	6	7,617	88.43	---	88.61
Above Sunrise	6	2,980	87.18	---	87.30
Sunrise	7	11,175	99.45	---	99.32
Sunrise	7	2,028	95.12	94.97	94.75
Sunrise	7	1,040	94.45	---	93.78
Sunrise	7	2,980	96.04	---	95.74
Rossmoor	3	4,039	91.76	91.89	91.86
Rossmoor	3	3,116	91.48	---	91.26

Comment 30: You state: "The most likely explanation for the performance of the 2-D model for this transect was that the measurements for the two banks at the calibration flow had been reversed." This does not explain the model's performance. It does explain the difference between predicted and (apparently) observed WSL values. If you can clearly attribute this to a data entry error, why not simply note that an error had occurred and the bank values were reversed. After all, this is not a deficiency of the model. (Page 10, Paragraph fragment at top)

Response: We were not confident that there actually was a data error. We have rewritten the sentence to make it clear that the model likely performed adequately.

Comment 31: An additional cause of point velocity discrepancy: The 2-D model integrates effects from the surrounding elements at each point. Thus, point measurements of velocity can differ from simulated values simply due to the local area integration that takes place. (Page 10, Paragraph 1)

Response: The above has been added to this paragraph in the report.

Comment 32: Change "fixed" to "overcome" and add "extended" in seventh sentence (Page 10, Paragraph 2).

Response: The suggested changes have been made.

Comment 33: Change "we had to work with" to "limitation" in last sentence. Avoid preposition at end of sentence. (Page 11, Paragraph 1).

Response: The suggested change has been made.

Comment 34: Change "hydraulics" to "hydrodynamics" in first sentence. Delete second, fourth and fifth sentences. Change "cdg file" to "discharge" in third sentence. (Page 11, Paragraph 2).

Response: We feel that it is important to include the details in the second, fourth and fifth sentences so that readers can understand the steps taken in the production step. The remaining suggested changes have been made.

Comment 35: If anything, this study shows equally large #'s of redds in areas of low suitability for 1+2D. Are the HSC adequate? (Page 12, Paragraph 1).

Response: We believe that the HSC are adequate. We have added figures to the report showing the depth and velocity HSC relative to frequency distributions of the redds - these indicate that the HSC fit the redd data fairly well, with discrepancies due to a bias in the redd data towards shallow and slower conditions as a result of low visibility during redd data collection - text to this effect has been added to the report. We feel that the large numbers of redds in areas of low suitability (Figures 3 and 6) reflect errors in the models' estimation of depth, velocity and substrate, and thus combined suitability at redd HJC → locations. In contrast, when the measured depths, velocities and substrates at the redd locations are used to calculate combined suitability, as in Figure 5, there are not a large number of redds in areas of low suitability, indicating that the HSC are adequate.

Comment 36: Delete "decks" and change "files" to "models" in second sentence. Add "for the discharge observed in Nov and Dec, 1998" to third sentence. Reference some statistics book and add "significantly" in the sixth sentence. (Page 12, Paragraph 3)

Response: Variations on the suggested changes have been made.

RESULTS

Terry Waddle

Comment 1: Change "deck" to "data set" in the fourth sentence. Add a sentence at the end that acknowledges the habitat-flow relationships were essentially the same. The variation can largely be attributed to the two methods of filling in the space between transects, assumed uniform extrapolation and explicit simulation. Variation is well within the uncertainly of estimate of your HSC. (Page 13, Paragraph 1)

Response: The suggested changes have been made.

Comment 2: The appendices contain extensive plots of measured and 2-D simulated velocities. Yet, the results section has very little discussion of the quality of fit of the predicted and observed values. The results section should be expanded to discuss the quality of velocity prediction. Further, the velocity predictions from the 1-D simulation should also be included. It should be noted that the area integration effect noted above will produce somewhat smoother lateral velocity profiles than the observations. (Page 13, Results Section)

Response: There was a fairly detailed discussion of the quality of fit of the predicted and observed velocities in the Methods section (on pages 10 and 11). Accordingly, we do not feel that it is necessary to add a discussion on this topic in the results section. The graphs in Appendix C include results of the 1-D simulation in cases where we collected a velocity set at a higher flow. We did not feel that it would be warranted to plot velocities simulated by PHABSIM at the same flow that the velocities were measured, since the PHABSIM-simulated velocities in that case would be virtually identical to the measured velocities as a result of how velocities are simulated in PHABSIM. We have added text to the effect that the area integration effect will produce somewhat smoother lateral velocity profiles than the observations.

Comment 3: Ie. more redds where suitability was zero? (Figure 3)

Response: There were more PHABSIM cells which contained redds where PHABSIM predicted the suitability was zero than cells with redds where PHABSIM predicted the suitability was non-zero. There were only four redds where the combined suitability calculated from the measured depth, velocity and substrate were zero, as shown in Figure 5.

Comment 4: Awfully uniform. It your HSC were correct, should the frequency ascend with suitability? (Figure 5)

Response: The frequency distribution can be attributed to three factors: 1) the difference in the distribution of depths and velocities of redds in 1998 versus the redd measurements used to develop the criteria; 2) the fact that combined suitability is calculated as the product of the depth, velocity and substrate suitabilities (i.e., if the depth, velocity and substrate suitabilities were all 0.8, the combined suitability would be 0.5; and 3) how fast the depth and velocity suitabilities drop off from 1.0. With regards to the last factor, nonparametric confidence limit criteria (which would have a suitability of 1.0 for depths of 1.4 to 2.8 feet, and a suitability of 1.0 for velocities of 1.6 to 2.8 ft/s), would have resulted in 27% of the 1998 redds having a combined suitability of 1.0. In contrast, the criteria used in this study (smoothed exponential) had suitabilities that dropped off to 0.77, 0.78 and 0.67, respectively, for a depth of 1.4 feet and velocities of 1.6 and 2.8 ft/s. Because of the last two above factors, we would expect that criteria developed using exponential smoothing would generally result in a frequency distribution of combined suitability that was relatively uniform. Thus, despite criteria that were "correct," in terms of having a good fit to the frequency distributions of the data used to develop the criteria, the frequency distribution of combined suitability was fairly uniform.

Comment 5: Just about as bad as PHABSIM (Figure 6)

Response: We agree. This comment is consistent with the report's text stating that the 2-D model did a slightly better job of predicting the combined suitability of redd locations.

Comment 6: Better than PHABSIM. Your HSC seem better for rejecting a location than for accepting it. (Figure 7)

Response: We agree that the 2-D model did a better job than PHABSIM at predicting low suitabilities for locations without redds. We disagree, however, that the HSC are better for rejecting a location than accepting it. Figure 5 provides a better indicator of the ability of the HSC to predict the suitabilities of redd locations. The distribution of combined suitabilities in redd locations in Figures 3 and 6 are mostly a reflection of errors in the predictions of depths, velocities and substrates by PHABSIM and the 2-D model, rather than reflecting the ability of the HSC to predict suitability of redd locations.

Comment 7:. The previous paragraph talks about 2-D model under-predicting velocities, yet the figure appears to show predicted (the heavy line) greater than observed for most points. Either the figure needs to be changed or the paragraph statement changed to "over-predicting." Predicted is heavy line, right? Make it heavier still or dash one or something. (Page 18, Figure 8)

Response: We assume that the commenter meant water surface elevations rather than velocities. The commenter is correct that the figure in the draft report showed predicted greater than observed for most points - the legends for the two lines had been reversed, and are corrected in the final report to show that the measured water surface elevations were in fact greater than the predicted for most of the site. The measured line has been made heavier in the figure so that it can be more readily differentiated from the predicted line.

REFERENCES

Terry Waddle

Comment 1: Now you can reference the DOC & Web site to reference the model.

Response: References, including web sites, have been added for the user's manuals for River2D, River2D_Bed and R2D_Mesh.

Comment 2: Also ought to reference Ashrat Ghanem's PhD Thesis as it gives the basis for the current form of the model.

Response: We have added an alternative reference (Ghanem et al 1995), which provides the same information (Peter Steffler, personal communication).

Comment 3: Reference for ADCP & its software.

Response: A reference for the ADCP and its software has been added.

APPENDICES

Terry Waddle

Comment 1: Many plots have the same labels so one cannot distinguish what is being displayed. The content of the plots clearly suggests differences in observed and simulated discharge but the labels do not reflect those differences. (plots page 32 and on)

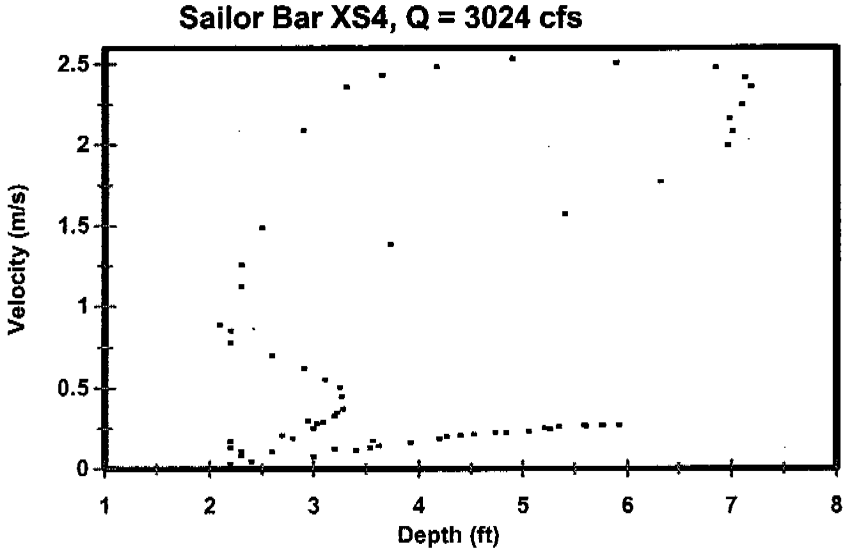
Response: Each plot has been relabeled and an index map has been added for each site showing the location of each plot.

Comment 2: why spike? I wonder if spike propagates. (Page 32, Sailor Bar XS1 figure)

Response: The spike is associated with inflow at the outflow boundary. The spike appears to only propagate 30 m upstream of the outflow boundary. The spike is probably caused by a local instability in the model, possibly associated with a split channel at the outflow boundary. The measured data also showed inflow at this portion of the outflow boundary, but at a much lower magnitude.

Comment 3: 2D vel proportional to depth? Inflow @ outflow boundary forces this distribution @ XS1? Extend site use BC5 & calibrate. (Page 32, Sailor Bar XS4 figure)

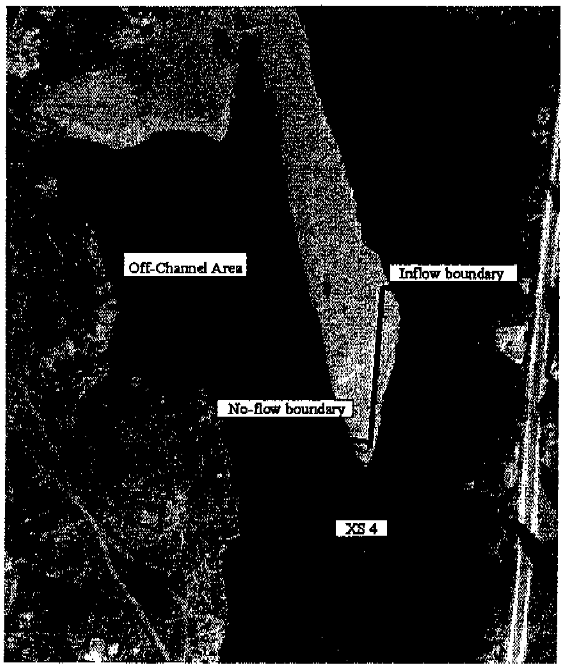
Response: We assume that the commenter meant XS4 instead of XS1. As shown in the graph below, the velocities simulated by the 2-D model do not appear to be proportional to the measured depths. Since the inflow at the outflow boundary only propagates 30 m upstream of the outflow boundary, it



does not appear that the inflow at the outflow boundary forces this distribution at XS4. Accordingly, we do not feel that it is necessary to extend the site downstream, use boundary condition 5 and recalibrate. In any case, time and manpower constraints make it impossible for us to recalibrate and rerun the Sailor Bar site model. As discussed in the report, we attribute the simulated velocity pattern at XS4 to the effect of the velocity distribution at the upstream boundary.

Comment 4: Sailor inflow at outflow boundary affects last 100 m velocity distribution, maybe more. Right Bank groove picks up too much Q. Inflow notched topography means invalid until distributes about 2 widths from notch. What's really going on? Rerun with full width inflow extended and outflow extended to remove distribution problems and outflow boundary errors. (Page 33)

Response: At the validation velocity flow for XS1 (3,042 cfs), the inflow at the outflow boundary only affects the last 30 m of the velocity distribution. Even at higher flows (10,287 cfs), the inflow at the outflow boundary only affects the last 50 m of the velocity distribution. Only the simulated velocities 35 m upstream of the inflow at the outflow boundary (shown in Sailor Bar Deep Beds A in the final report) and at XS1 differ substantially from the measured velocities due to the inflow at the outflow boundary. The effect of the inflow at the outflow boundary does not appear to affect the velocities 100 m upstream of the outflow boundary (shown in Sailor Bar Deep Beds B in the final report). We agree that the right bank groove does pick up too much flow - we attribute this to the velocity distribution at the upstream boundary. The notched topography at the inflow boundary represents the real configuration of the river channel upstream of the Sailor Bar site - the notched area is an entrance to an off-channel area (as shown in the aerial photo below), with a resulting major expansion of the river channel at this location. Time and manpower constraints make it impossible for us to modify and rerun the Sailor Bar site model.



Comment 5: Which Xsecs in graphs, labels all the same (Page 33)

Response: The graphs do not show cross-sections, but instead show the locations between cross-sections where bed topography and velocity data was collected with the ADCP. Each plot has been relabeled and a an index map has been added for each site showing the location of each plot.

Comment 6: How much of this scatter is due to inflow on outflow boundary. (Page 36)

Response: Very little of this scatter is due to inflow on the outflow boundary. Only XS1 and the lower-most ADCP run (approximately 30 m upstream of XS1) were affected by the inflow at the outflow boundary, since the effect did not extend up to the second-lower-most ADCP run (approximately 100 m upstream of XS1). Further, only 16 points on XS1 and 24 points on the lower-most ADCP run were affected by the inflow at the outflow boundary, for a total of 6% (40/695) of the validation velocity measurements.

Comment 7: Outflow boundary anomaly, extend. Lower flows all need extension, eddy forms near/on boundary. (Page 40)

Response: We do not feel that it is necessary to extend the outflow boundary because of the anomaly at the outflow boundary, since the anomaly only covers a 2 m^2 area. While the eddy at the lowest flow near the downstream boundary is substantial in size (around 42 m^2), the compound suitability in this area is zero, and thus an extension of the outflow boundary is not necessary. In any case, time and manpower constraints make it impossible for us to modify and rerun the Above Sunrise site model.

Comment 8: For lower Q's have split channel @ outflow. Much better to extend with artificial topography and let model distribute flow without relying on fixed WSL or on $q_s = ad^b$. (Page 43)

Response: The commenter is correct that there is a split channel at the outflow boundary at lower flows. While we agree that it would be better to extend the site downstream with artificial topography, time and manpower constraints make it impossible for us to modify and rerun the Sunrise site model.

Comment 9: Give velocities for same Q in split channel. (Page 43, Sunrise XS1 figures)

Response: We were unable to do so because there was essentially no flow in the side and right main channels at 3,114 cfs and we did not measure velocities in the left main channel at 4,039 cfs.

Comment 10: Page 45 to here all have same name, same Q yet appear quite different. Explain. (Page 49).

Response: Each graph shows a different location where bed topography and velocity data was collected with the ADCP. Each plot has been given a different label and a an index map has been added for each site showing the location of each plot.

Comment 11: Eddy on outflow boundary - extend. Extended inflow [boundary] enough. (Page 52)

Response: While the commenter is correct that the model shows an eddy on the outflow boundary, time and manpower constraints make it impossible for us to modify and rerun the El Manto site model. It should be noted that the measured data also showed an eddy on the outflow boundary.

Comment 12: Circled may be only valid velocities in measurement as Q measured > Q simulated and next Xsec matches. (Page 60, upper left-hand graph)

Response: The commenter may be correct. The data in question (shown in the final report as Rossmoor Deep Beds G) was collected using Mode 4 of the ADCP - Mode 4 tends to be less accurate than Mode 8. This ADCP run covered most of the channel, so the discharge calculated from this ADCP run should be less than the actual discharge of 4,043 cfs. In fact, the discharge calculated from this ADCP run is 4,965 cfs, which indicates that the error in discharge is greater than 23%. We have found that if we collect three ADCP runs and use Mode 8 when the average velocity is less than 1.78 m/s, or Mode 4 when the average velocity is greater than 1.78 m/s, the discharge measured with the ADCP is usually within 5% of the known discharge from gage readings. In this case, we were less concerned with the quality of the velocity measurements, since the main use of the ADCP data was to determine the bed topography of the site.

Comment 13: Here Q as $\int_{360}^{385} V_i$ appears to match for measured and simulated. (Page 60, upper right-hand graph)

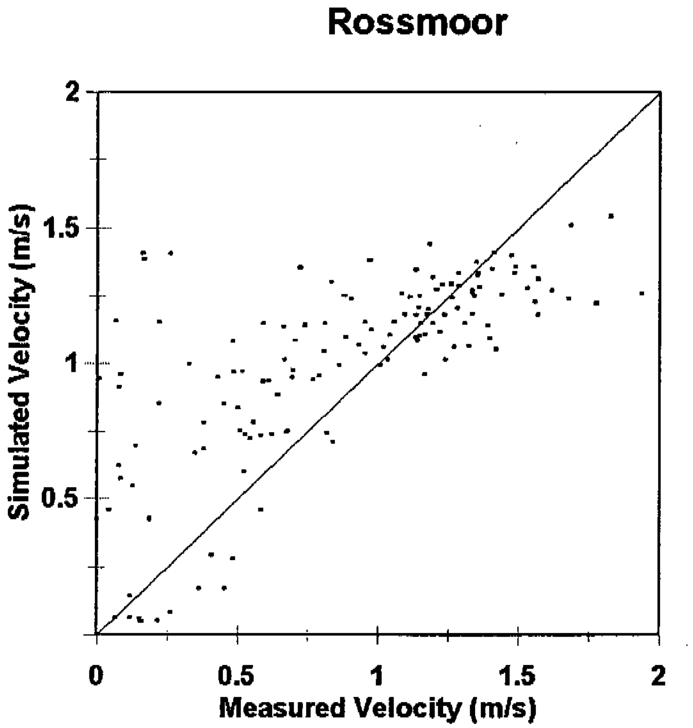
Response: We agree. However, this ADCP run (shown as Rossmoor Deep Bed I in the final report) covered less than half of the river channel.

Comment 14: Again suspect measurements. (Page 60, lower left-hand graph)

Response: This ADCP run (shown as Rossmoor Deep Bed K in the final report) covered less than half of the river channel and may reflect an error in the 2-D model's distribution of the flow across the channel rather than an error in the ADCP data. Also noteworthy is that this ADCP run was made using Mode 8, which, as discussed above, would be expected to be more accurate in velocity measurements.

Comment 15: How about for Xsecs where Q matches? (Page 60, lower right-hand graph)

Response: The graph below shows measured versus simulated velocities for the ten deep bed runs at Rossmoor where Mode 8 (which would be expected to give more accurate velocities) was used (Deep Beds H through Q in the final report). There does appear to be a better correlation between measured and simulated data when only Mode 8 data are examined.



Comment 16: As a side note, I also question some of the variation in the observed velocity profiles. It would be worthwhile to string a cable somewhere and take a good set of long averaged measurements followed by 10 passes with the ADCP to see how much averaging of the ADCP values is needed to produce the bomb and current meter measurements. Right now, you do not have a good estimate of the amount of noise in the ADCP results. Clearly for some transects, the integral of velocity across the stream would give much different discharge than the integral of the simulated velocity profile. Because the model enforces mass balance, I have trouble believing some of the ADCP velocity profiles that have a much larger enclosed area than the simulated velocities.

Response: We have conducted a similar type of test by comparing discharge calculated with the ADCP versus known (gage) discharges, as discussed in the response to comment 12. Based on this data, it is safe to say that there is considerably more noise in ADCP data collected with Mode 4 than with Mode 8. However, Mode 8 does not work in shallow, fast water, so we have had to use Mode 4 for some of the ADCP data collection. Overall, on the American River for the ADCP data collected in between transects, Mode 4 was used for 19 runs (mostly for Rossmoor and El Manto sites), while

Mode 8 was used for 56 runs. In addition, Mode 8 was used for collecting deep velocities on 18 transects, while Mode 4 was used for collecting data on the remaining nine transects. For the transect data, all of the measured discharges fell within 8% of the known (gage) discharge, while only three transects had measured discharge that differed by more than 5% from the known discharge. As noted in the report, we attribute much of the difference between measured and simulated velocities to noise in the velocities measured by the ADCP.

SIMULATION RESULTS PROVIDED ON CD ROM

Terry Waddle

Comment 1: Inflow occurs at the outflow boundary. This affects velocities in at least the last 100m of the study site. The deep channel along the south west bank conveys more discharge than the observed velocity profile would indicate. Attempt [to] remedy these situations by artificially extending the topography upstream and downstream 1 to 3 channel widths. At the downstream boundary, extend the channel until the eddy that is forming on the boundary is entirely encompassed in the extended topography. This will both allow space for the eddy to act and for it to dissipate. (Sailor Bar)

Response: At the validation velocity flow for XS1 (3,042 cfs), the inflow at the outflow boundary only affects the last 30 m of the velocity distribution. Even at higher flows (10,287 cfs), the inflow at the outflow boundary only affects the last 50 m of the velocity distribution. Only the simulated velocities 35 m upstream of the inflow at the outflow boundary (shown in Sailor Bar Deep Beds A in the final report) and at XS1 differ substantially from the measured velocities due to the inflow at the outflow boundary. The effect of the inflow at the outflow boundary does not appear to affect the velocities 100 m upstream of the outflow boundary (shown in Sailor Bar Deep Beds B in the final report). We agree that the deep channel along the south west bank conveys more discharge than the observed velocity profile would indicate - we attribute this to the velocity distribution at the upstream boundary. The upstream boundary was already artificially extended one channel width upstream. Time and manpower constraints make it impossible for us to modify and rerun the Sailor Bar site model.

Comment 2: At the upstream boundary, create an initial channel configuration proportional to the observed velocity distribution starting with a 1 channel-width extension and extending further if needed. It may also be necessary to identify why the backwater effect is keeping velocities so low on the south west side. Higher roughness in this area may have some effect. (Sailor Bar)

Response: We agree that creating an initial channel configuration proportional to the observed velocity distribution could improve the simulation of velocities for this site, and have already stated so in the report. We assume that the commenter is referring to the northwest side of the upstream boundary in the second portion of this comment. Velocities are actually low in this area due to an off-channel area immediately upstream of this area (see response to Appendices Comment 4). It might be possible, however, to also address the low velocities on the northwest side of XS4 by creating an initial channel configuration proportional to the observed velocity distribution. However, time and manpower constraints make it impossible for us to modify and rerun the Sailor Bar site model.

Comment 3: How much of the scatter in the plot on page 36 is due to inflow at the outflow boundary? (Sailor Bar)

Response: Very little of this scatter is due to inflow on the outflow boundary. Only XS1 and the lower-most ADCP run (approximately 30 m upstream of XS1) were affected by the inflow at the outflow boundary, since the effect did not extend up to the second-lower-most ADCP run (approximately 100 m upstream of XS1). Further, only 16 points on XS1 and 24 points on the lower-most ADCP run were affected by the inflow at the outflow boundary, for a total of 6% (40/695) of the validation velocity measurements.

Comment 4: Best fit of lateral velocity profiles and scattergram. However, this site would also benefit from a downstream extension so velocity variation at the downstream boundary could be more fully worked out by the model. At the lowest discharges there is inflow at the outflow boundary so an extension is in order here also. (Above Sunrise)

Response: While we agree that it would be better to extend the site downstream with artificial topography, time and manpower constraints make it impossible for us to modify and rerun the Above Sunrise site model. While the inflow at the outflow boundary at the lowest flow affects a substantial area (around 42 m^2), the compound suitability in this area is zero, and thus an extension of the outflow boundary is not necessary.

Comment 5: For the lower discharges, there is a split channel at the outflow boundary. It is much better to avoid this condition and allow the model to set exit WSL rather than conform to a fixed value. This is best accomplished by extending the site with artificial topography using breaklines to connect the real and arbitrary section. The specific discharge boundary condition, $q = a*d^b$ can be employed, but calibration of the coefficients to produce accurate WSL values in both channels will be difficult. (Sunrise)

Response: While we agree that it would be better to extend the site downstream with artificial topography, time and manpower constraints make it impossible for us to modify and rerun the Sunrise site model.

Comment 6: There is one no flow boundary segment in the middle of the outflow boundary cross section. This causes one anomalous velocity at that boundary. While this does not have a significant effect on the overall outcome of the study, such subdivision of the outflow boundary is not recommended. A better choice is to extend the site so no flow split occurs on the boundary. (Rossmoor)

Response: While we agree that it would be better to extend the site downstream with artificial topography, time and manpower constraints make it impossible for us to modify and rerun the Rossmoor site model.

Comment 7: There is an eddy in the southwest corner at the outflow boundary. Extend the study site to avoid an eddy on the boundary. This eddy is of small magnitude and will not influence much of the study area. However, if the southwest corner of the study site was a good spawning area then it may affect habitat outcomes. (El Manto)

Response: While we agree that it would be better to extend the site downstream with artificial topography, time and manpower constraints make it impossible for us to modify and rerun the El Manto site model. The portion of the site with the eddy would not be a spawning area, since the substrate in that location has a suitability of zero.